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The author endeavors to penetrate the "mists of mentalistic myth" which enshroud the "very real, solid, and substantial results of generative or transformational linguistics." In attempting to clarify and clear up misunderstandings about theories of grammar as put forth by Chomsky (whose practice, the author feels, is "superior to his description of it"), the author discusses what he considers to be essential points of agreement between Chomsky and Bloomfield. He states that there is and can be no such thing as "the (only) correct grammar even of the English of some limited linguistic group at some particular time." Grammars should be treated as scientific theories "since it is only with this limitation that mentalism in linguistics has any plausibility whatsoever." There is nothing in linguistic practice which should lead to the belief that the internal structure and operation of a grammar is in any way identifiable with, or a duplication of, the internal structure or operation of a mind or a brain. A grammar represents, ideally, the output of the speaker-hearer-judge. This paper will appear in "Thought and Language: An Interdisciplinary Symposium," to be edited by J.L. Cowan and published by the University of Arizona Press, Spring 1969. (AMM)

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J. L. Cowan

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I. Introduction.

II. Notes on the Contributors.

III. Charles E. Osgood: Interpersonal Verbs and Interpersonal Behavior.

IV. George Mandler: Words, Lists, and Categories: An Experimental View of Organized Memory.

V. Rulon Wells: Comprehension and Expression.

VI. Zeno Vendler: Say What You Think.

VII. Paul Ziff: Understanding.

VIII. Joseph L. Cowan: The Myth of Mentalism in Linguistics.

THE MYTH OF MENTALISM IN LINGUISTICS

by J. L. Cowan

> If you wish to learn from the theoretical physicist anything about the methods which he uses, I would give you the following piece of advice: Don't listen to his words, examine his achievements. For to the discoverer in that field, the constructions of his imagination appear so necessary and so natural that he is apt to treat them not as the creations of his thoughts but as given realities.
>
> A. Einstein

> As physics goes so goes linguistics.
>
> J. L. Cowan

Much of the excitement generated by the MIT school of linguistics has arisen from the explicit and repeated challenges apparently flung by the various members of that school in the faces of so many of the rest of us. The methods and results of the structural linguistics dominant on the continent, the British Firthian and neo-Firthian schools, and perhaps above all the taxonomic approach of pre-MIT American linguistics have all been subjected to these challenges. Nor have even such ancillary disciplines as psychology and philosophy been spared. Psychologists have

repeatedly been castigated over the sterility which must inevitably result from ignorance on their part of the very latest, and indeed usually still unpublished, conclusions of the new linguistics. The great bulk of the apparatus of method and theory, especially in the area of learning, painfully wrought by psychologists over the century or so prior to the publication of Syntactic Structures in 1957 and still dominating the field has thus now been called in question. The very empiricism which as a general epistemological position and as an absolutely essential element in the understanding of science has been accepted by the vast majority of Anglo-American philosophers and scientists--not, I hope, as dogma, but certainly as eminently well secured philosophically--has itself been held refuted by these new results as the challenging title Cartesian Linguistics itself indicates.

Now the thesis of this paper is fairly simple and quite straightforward. I shall argue that the very real, solid and substantial results of generative or transformational linguistics have come to us clothed in a complex set of metaphors so firmly interconnected and so well integrated as fully to deserve to be called a "myth," to be called, in fact, from its most central or core metaphors, "the myth of mentalism."

Noam Chomsky initiates his recent book Aspects of the Theory of Syntax with statements such as the following:

> Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance... To study actual linguistic performance, we must consider the interaction of a variety of factors, of which the underlying competence of the speaker-hearer is only one. In this respect, study of language is no different from empirical investigation of other complex phenomena. We thus make a fundamental distinction between <u>competence</u> (the speaker-hearer's knowledge of his language) and <u>performance</u> (the actual use of language in concrete situations)... The problem for the linguist, as well as for the child learning the language, is to determine from the data of performance the underlying system of rules that has been mastered by the speaker-hearer and that he puts to use in actual performance. Hence, in the technical sense,

> linguistic theory is mentalistic, since it is concerned with discovering a mental reality underlying actual behavior. Observed use of language or hypothesized dispositions to respond, habits, and so on, may provide evidence as to the nature of this mental reality, but surely cannot constitute the actual subject matter of linguistics, if this is to be a serious discipline. (1965, pp. 3-4)

That we are dealing here with a set of metaphors seems undeniable. The validity of modern linguistics does not rest on the truth of Platonism one might be tempted to say except that even this would be misleading since the "Platonism" even of Plato consists largely in the deployment of much the same metaphors equally metaphorically to comprehend much the same range of facts. There is no such ideal speaker-listener or speech community, and the much ado of recent linguistics, whatever it may be about, is most certainly not about nothing.

Now the metaphors which compose this myth are, like most metaphors, not without their positive values. They present in a powerfully compressed and tightly organized fashion a whole mass of sloppy and initially confused facts. This myth has certainly played a great part in the brilliance of style, tone and direction which has characterized the MIT school. In spite of certain

misleading consequences which I shall point out, the myth may even have had some heuristic value. It is therefore with some reluctance that I turn to the tedious, pedestrian--even Philistine--task of unpacking some of these metaphors, of unearthing for inspection some of the grubby facts which the beautiful myth of mentalism has served to clothe and thus to hide, of twitching the gorgeous mantle of linguistic fancy from the knobby knees of linguistic fact. It is my hope that the sacrifice of aesthetic values incurred in this painful process will be compensated for by the gain in understanding. For the second portion of my thesis is that the greater part of the challenges referred to above spring not from the actuality but from the appearance, not from linguistic reality but from the mentalistic myth in which that reality has been conveyed to us.

Let us then begin at, or at least with, the end. What is the purpose of the professional output of the linguist? For specificity let us take the example of a grammar of English. As is usually done in such discussions, I will have syntax uppermost in mind, but my remarks will also be applicable with slight changes to phonology, to semantics, and to the broader theory in which a grammar containing these three components might have its place. What is the purpose of such a grammar? As soon as this question is explicitly raised the answer becomes obvious. Grammars have no purposes at all. They are not the sorts of things that could have purposes. They simply sit there on the pages of notebooks

or professional journals or monographs or texts, not, certainly, content with their lot, but just as certainly not projecting directions of movement or maintenance for the future. Nor does even linguistics as an institution have purposes let alone a purpose. It is people who have purposes and both people and purposes in the plural at that. There are many different purposes which may go into the making of a grammar, and many different purposes for which a grammar once made may be employed.

One might, for example, want a grammar to be a scientific instrument of the greatest power and accuracy, capable of saving the appearances, of handling one or another body of data down to the fine detail. But one might, on the other hand, be interested primarily in grammar as a pedagogic instrument to be used in instructing one or another group of learners of the language in question. One might be concerned, on the one hand, with the constructibility of one's grammar, wanting something which could be arrived at in a fairly or quite mechanical way from a body of data of one or another kind. Or one might, on the other hand, be more concerned with the fairly or quite mechanical application or operation of the grammar. One might be concerned with one's grammar simply as the most efficient instrument for dealing with one particular language, or even some part of a language only.

Or one might be concerned to observe certain general constraints such as might be imposed by taking into account such factors as supposed linguistic universals. One might be content with a synchronic grammar or one might want to build in parameters of evolutionary change. One might want a grammar for descriptive purposes of one sort or another or again for one sort or another of prescriptive purposes--for in spite of current fads in linguistics there are no stronger reasons against prescription with respect to language than there are against prescription with respect to the movement of vehicular traffic, and probably at least as strong reasons for. Since this brief sample can be added to indefinitely, since each item is itself indefinitely subdivisible, and since an indefinite number of combinations of items and sub-items is possible, such aims are quite literally innumerable.

It is, moreover, equally obvious that the grammar most suitable for obtaining one of these ends will often fail to be that most suitable for attaining another. Why should we suppose, for example, that the grammar which is scientifically most precise in one or another sense will also be the instrument pedagogically most effective in one or another sense? Why should we suppose that the most efficient grammar for improving the English of Headstart pre-kindergardeners in Detroit will also be the most

efficient for teaching English to Turkish businessmen with no previous exposure to the language? Why should we suppose, indeed, that the most efficient instrument for businessmen in Ankara will also be the most efficient for those in Gaziantep?

Consider only the question of generative grammar. Surely one of the great contributions of the MIT school has been the increased application to linguistics of the conceptions of rigor and precision developed by modern logic and mathematics. The operation of a truly generative grammar is to be mechanical or effective in the technical sense of logic. As Chomsky puts it, such a grammar "does not rely on the intelligence of the understanding reader..." (1965, p. 4) But as these terms are being used in the technical sense of logic, we have already here a sort of metaphor with respect to their ordinary acceptations. Valuable as rigor and precision and even as effective or mechanical operations are in some respects and for some purposes, they are by no means a be-all or an end-all. The fact that a comparatively stupid and unimaginative computer might be programmed to operate a grammar does not necessarily help you or I to do so. Thus a college freshman, for example, as opposed to a computer, might very well find more helpful an instrument which required for its application an intelligence he could muster, than he would find helpful one

which, while it did not require even this modicum of intelligence to apply, required far more intelligence than he could bring to bear in the time available to master it in the first place. A good analogy here is logic itself. If the time of students in an introductory course in logic is taken up with presentation of the propositional calculus and a reasonably complete version of quantificational logic, the brighter among them will attain some facility in formal manipulations. But almost none of them will be able to do as good a job of the analysis of ordinary, non-formalized arguments as they would have had they been given instead a simpler and in some ways inherently less complete and adequate formal machinery but more practice in applying it. Very, very few are the scientific theories which more than approximate the logician's ideal of explicitness, of rigor, of precision and of full logical articulation. Most such theories are far closer to violins and oboes in the skill required of their operators than they are like good computer programs. Even mathematics in some of its most creative episodes has fallen far short of the modern ideals of rigor, and the fact that most of the probings and many of the proofs which led to our most significant mathematical results leave much to the intelligence, insight and intuition of their readers make them defective in some respects, but all the same time more effective in others.

All this being the case it would seem inevitably to follow that there is and can be no such thing as the (only) correct grammar even of the English of some limited linguistic group at some particular time. And as this is so I should like to steal a bit from Carnap on logical syntax and propose for syntax in general and even more broadly for all linguistics a Principle of Tolerance:

> It is not the business of linguists to set up prohibitions but to arrive at linguistic descriptions which may quite legitimately be at least as diverse and various in their characteristics as they are in their potential applications. There are no morals in linguistics.

Now what I have been saying thus far seems rather obvious to me. I hope it is equally obvious to my readers since I have refrained on the assumption that it was obvious from more than beginning to move to its support the mountain of evidence available for that task. It has been necessary to mention it, however, since it has not by any means been always uppermost in the minds of all linguists and commentators thereon. Thus we find Katz in "Mentalism in Linguistics" telling us that "the aim of theory construction in linguistics is taken to be..." (1964, p. 128); Katz and Postal in An Integrated Theory of Linguistic Descriptions telling us that

"a linguistic description of a natural language is an attempt to reveal..." (1964, p. 1); Chomsky in Syntactic Structures asserting that "Syntax is the study of the principles and processes by which sentences are constructed in particular languages. Syntactic investigation has as its goal the construction of a grammar that can be viewed as a device of some sort for producing the sentences of the language under analysis, " (1957, p. 11) and in Aspects of the Theory of Syntax, "A grammar of the language purports to be..." (1965, p. 4) If my principle is accepted, then, a fair number of statements which actually read "The purpose, aim or goal of syntax, grammar, linguistic theory and so on is..." will have to be understood as meaning "A or one legitimate purpose, aim or goal is..."

I should like to suggest, moreover, that it is the very prevalence of the mentalistic myth which has for these linguists obscured the obvious. The Bloomfieldian approach, while in its over-restrictive conception of scientific theories being to some extent anti-tolerance was basically pro-tolerance in its emphasis on the artificiality in the non-pejorative sense, the artifactness, of linguistic results. When, however, one envisages grammar as somehow written on the soul or inscribed in the brain of the speaker-hearer and conceives of description as some kind of exact duplication of the object described, it is more difficult to retain the balance tolerance requires.

In the remainder of this paper I shall treat exclusively of grammars considered as scientific theories since it is only with this limitation that mentalism in linguistics has any plausibility whatsoever. It has been necessary to supply the foregoing considerations, however, both because they are intrinsically important and so frequently ignored in recent discussions, and because they bring out clearly the narrowness of even the greatest possible domain of mentalism in linguistics.

If, then, the linguist's output is to be considered a scientific theory as opposed, say, to a piece of pure logic or mathematics, it must be connected with, must be a means of organizing, some kind or kinds of observable data. Presumably, if our scientific theory is to be linguistics at all, these data should have something to do with language; but this still leaves a very great deal of leeway. We might, for example, limit ourselves to actual stretches of discourse, utterances spoken or written. There is and can be nothing inherently wrong with such a procedure nor with the grammars which might result from choosing it. The subscribers to the mentalistic myth will of course say that these data of performance result not from the operation of linguistic factors alone, but from these plus innumerable others which are linguistically irrelevant. But this response indicates only a

difference in a choice of ranges of data and types of theory to employ in handling these data. We are perfectly free to follow such prescriptions or to reject them. Even in linguistics as scientific theorizing there are no morals.

But just as there is no a priori reason why we should not limit ourselves to such data, so there is no a priori reason why we should. We may also utilize, in particular, as linguists almost universally do when possible, judgments on the part of those who have learned the language as to what is or is not a grammatically acceptable sample of it. As these are the chief types of data actually used and discussed by linguists, I shall confine myself to them, merely pointing out in passing that just as there can be no a priori ground for excluding either of these, so there can be none of excluding data of still further kinds.

A number of points must be remarked even about these two kinds of data however. It must be noted, first, that there is nothing mentalistic, not to say spiritualistic, about data of the second kind, judgments of grammaticalness, any more than there is in those of the first kind. One factor leading to the failure to see this is that these judgments are often said to be "intuitive," to express the "linguistic intuition," of their subjects. So they are and do. This, one perfectly good sense of the terms 'intuition' and 'intuitive,' simply means that the judgments are not derived from any

consciously articulated linguistic theory or grammar. In spite of the fact that MIT, in accord with its myth, uses the term 'performance' as a technical term for data of the first kind only, for actual samples of discourse, actually such judgments are also performances in every bit as good a sense of the term as it is ordinarily used. They are perfectly legitimate pieces of behavior in their own right and as such are perfectly unexceptionable pieces of data for behavioral theory.

This is an extremely important point. Just as failure to grasp it led some earlier linguists to hold too low an opinion of judgmental data in theory if not in practice, so failure to grasp it has led the new linguists, in theory if not in practice, to hold too high an opinion of them. Thus Chomsky is extremely misleading, for example, in speaking of a "lower" level of success achieved by a theory which accomodates utterance data correctly and a "higher" level of success if judgmental data are also accomodated. Neither kind of data is higher or lower than the other. Neither is deeper or more superficial. Neither explains the other. Judgments of grammaticalness are not the competence which, together with other factors, produces discourse. This might be the case if we spoke by selecting from a number of possibilities offered those sentences we judge grammatical, but in general this does not seem to be the way we do proceed. Both judgments and utterances

may rather be regarded, in a sense still to be explored, as different manifestations or results of competence--or, for that matter, of incompetence. Both of these kinds of data are thus, while different in kind, on the same basic level and both are perfectly good hard data.

A linguistic theory, then, may be regarded as a device for systematizing enormous amounts of such data actual or potential. A generative grammar, together with a lexicon, will enable us indefinitely to produce sentences all of which will hopefully be grammatical in the sense of being consonant with the data themselves. The grammar will provide a test of grammatical sentencehood for any proferred object. And for each grammatical sentence it will provide one or more structural descriptions which will indicate important relations between sentences and between their elements. The broader linguistic theory in which such a grammar is embedded may, then, in a sense still to be explored, explain or justify the grammar itself or proper parts of it.

What, then, is the cash value of speaking of such a theory as about the "competence" of an "ideal speaker-hearer?" What is actually meant by this, basically, is that linguistic theories, like other scientific theories, may with perfect propriety be regarded as having some life of their own and not merely as the slaves, let alone the creatures, of their data. Such a theory may be considered about an ideal because the language as defined by it need and almost inevitably will not correspond exactly with the language as defined by the actual data. If a theory does a

good job on the whole and for the most part we will not worry about a few deviations here and there. We will not be bothered by these particularly where we have or hope to have a supplementary theory or set of theories to account for them. This is the real burden of the competence-performance distinction. Chomsky says, as quoted above, "To study actual linguistic performance, we must consider the interaction of a variety of factors, of which the underlying competence of the speaker-hearer is only one. In this respect, the study of language is no different from empirical investigation of other complex phenomena." (1965, p. 4) He clearly does not mean to imply by this that he has discovered a strange entity called 'competence' which works in just the way his generative grammar does in the better, purer world which is its home, but which here below is corrupted and bedeviled and turned from its mark by brute matter, necessity and chance. The fact is that we have theories, some of them rather rough and ready, some fairly complex and sophisticated like the Freudian, which we can and do use <u>together</u> with our grammar to account more or less for the actual data. A paradigm here is mechanics in which we might account for a movement in terms of the interaction of, say, inertial and gravitational forces. We should certainly be aware however that the actual movement is actually a simple smooth parabola. This in turn is "composed"

of horizontal and vertical elements not in the sense that these independentally exist and are somehow even more real than their "resultant," but simply in that they are distinct elements in our equations. In employing the phrase "complex phenomena" as Chomsky does, we are not saying anything about the phenomena simply in and of and unto themselves, but merely referring to the fact that our treatment of them is accomplished through a complex theory or even a complex of theories. "Competence" is thus not an antecedent datum which justifies one conception of grammar over others, but is rather a consequence of adopting one such conception instead of others. This choice obviously cannot therefore be justified in terms of a competence-performance distinction. The choice will rather be justified only in so far as the total complex of theories successfully copes with the actual data of performance, and will even then be justified no further than alternative analyses which do the same job equally well.

We must recognize, then, that since neither of these two kinds of data is actually, as the myth would have it, based on or the direct outcome of the other, conflicts or incompatibilities between them might emerge. It might very well develop that a sort of thing judged grammatical was a sort of thing never actually said, or, conversely, that a sort of thing consistently said and inexplicable

by our ancillary theories as a deviation was consistently judged to be grammatically unacceptable. In actuality, as opposed to myth, we have no guarantee whatsoever against this sort of occurrence. Once the myth is abandoned, however, this sort of thing, while requiring more complexity in our theories, need bother us no more than discrepancies within each type of data such as that judgments may vary somewhat from subject to subject or from time to time.

The myth bemuses even further. To an extent the "speaker-hearer competence" metaphor conceals the full force of the "ideal" metaphor. Chomsky has noted that the characterizations of the data actually employed in linguistic practice present as well as past are rather rough and ready. Eliciting a judgment of grammatical-ness or of ambiguity, for example, is usually not just a matter of presenting a sentence in isolation. What is often done is to provide a context, to tell little stories as ordinary language philosophers do. It might even be suggested, in fact, that an ungrammatical sentence is simply one such that no one has yet thought of a suitable context in which it might be employed. Be that as it may, however, different operations of elicitation may clearly produce quite different sets of data. Chomsky of course is well aware of this but points out quite correctly that while more precise operational specifications of data elicitation may be necessary at some point

in the development of linguistics, the present problem is rather a lack of theories adequate to handle the superabundance of unquestionably adequate data already available. Uninhibited by the myth one could go even further than this. One might suggest that not only is it practically unnecessary further to develop operational specifications in the present absence of solid theory, but that it would be theoretically unwise to do so. One may expect that in linguistics as in other sciences the best procedure will be to develop data and theory reciprocally, to have the theory fit the data more or less, true, but also at least to some extent to define the data in accord with the developed theory.

Here again Chomsky's practice is superior to his description of it. His sharp distinction between grammaticalness and acceptability is clearly somewhat circular insofar as grammaticalness is defined by acceptability. What actually happens, of course, is that the linguist formulates rules based on some acceptable instances and then insists that other examples generated by these rules but not acceptable must be so on other grounds than grammatical ones. This slight circularity, which is obscured by the myth of grammatical rules as inscribed on the soul, would be better recognized as such. It is not, however, necessarily a vicious circularity, but, to judge

from the example of other sciences which have practiced this sort of thing, may be most virtuous.

There is one further area in which what is and what is not actually contained in the "ideal" and "competance" metaphors should be spelled out. One may expect that in linguistics, as in other sciences, theories will not always be mere shorthand summaries or representations of given data as was thought, or wished, by early positivists such as Mach. One fairly trivial reason for this is that such theories will generally employ universal variables. More significant is the fact that there is no essential reason for requiring theoretical terms to be explicitly definable in terms of observable data. Some kind of observational tests must be obtainable for a theory if it is to be an empirical theory at all. But it is certainly not necessary that either the theory itself or all the concepts in it be somehow reducible to any collection, even infinite, of such data. These matters have been discussed at such great length in recent years and are now so well understood and accepted by even the most empirically minded philosophers that I shall not dwell upon them further here.

What it is necessary to do here, however, is to point out that this freedom from referential function in any direct or simple sense on the part of such theoretical terms is indeed just that, a freedom from referential function. The fact that such terms do not

simply denote sets of empirical observables, pieces of behavior, for example, does not mean that they must therefore denote some other theory independent entity whether a supposedly intrinsically non-empirical-non-observable entity like unconscious mental states or a simply not-yet-intelligibly-observable-in-these-terms-entity like brain states. To suppose that such terms must have such a reference is to accept again that primitive, Neanderthal, theory of meaning as theory independent reference on which the errors of the early positivists, behaviorists, et al were themselves exactly based. To work essentially and well in a perfectly good empirical theory neither the expression 'psi function' nor the expression 'grammatical transformation' need designate an independently specifiable kind of entity--a bit spooky and inaccessible perhaps, but an entity still. Linguists, I must add, are by no means alone in misleading others, if not themselves, in these respects. A psychological theory, such as operant conditioning, which directly and relatively simply connects events outside the organism with behavior is called in a quite proper technical sense an "empty organism" theory. Thus when we find it necessary to complicate the simple connection between externals and behavior we may perhaps be said, in the technical sense, no longer to have an empty organism theory. But it is essential to realize that this technical sense

is by no means the ordinary sense. The emptiest organism of empty organism theory was already filled with blood and bone and nervous tissue. We do not by modifying our psychological theories fill it any fuller. To complicate a theory is not to populate an organism.

These, then, I should like to suggest, are the main and multifarious points meant and not meant literally by the mentalistic metaphor. Perhaps the central fact about which we should now be clear is this: There is nothing whatsoever in linguistic practice which should lead us to believe that the internal structure and operation of a grammar is in any way identical with, a duplication of, the internal structure or operation of a mind or a brain. A grammar represents, ideally, the output of the speaker-hearer-judge. Whether or not it also represents that which actually puts out that output is quite another question, and a question linguistics itself gives us no means even of formulating in intelligibly adequate detail, still less of answering. The new linguists iterate and reiterate that they are trying to represent the knowledge of the speaker-hearer, what he knows. This is perfectly true. But it does not mean that they are representing what is inside his head. What the speaker-hearer, knows is the language. Saying that linguists are representing what the speaker-hearer knows is therefore no more than a picturesque way of saying that they are representing the language.

One might as well say that astronomy is mentalistic since it represents what the ideal star gazer knows, or, to take a somewhat more precise analogy, that mechanics is mentalistic since what the physicist is trying to represent is what we all learn when we learn to walk.

Now that Chomsky is himself far more in control of and far less controlled by his metaphors than are many others is quite clear. Thus he tells us clearly and well in Aspects that:

> To avoid what has been a continuing misunderstanding, it is perhaps worthwhile to reiterate that a generative grammar is not a model for a speaker or a hearer. It attempts to characterize in the most neutral possible terms the knowledge of the language that provides the basis for actual use of language by a speaker-hearer. When we speak of a grammar as generating a sentence with a certain structural description, we mean simply that the grammar assigns this structural description to the sentence. When we say that a sentence has a certain derivation with respect to a particular generative grammar, we say nothing about how the speaker or hearer might proceed, in some practical or efficient way, to construct such a

> derivation. These questions belong to the theory of language use--the theory of performance. No doubt, a reasonable model of language use will incorporate, as a basic component, the generative grammar that expresses the speaker-hearer's knowledge of the language; but this generative grammar does not, in itself, prescribe the character or functioning of the perceptual model or model of speech production. (1965, p. 9)

What Chomsky is asserting here would seem to be in fundamental agreement with Bloomfield's own position as expressed in statements such as the following:

> We can describe the peculiarity of these plurals [knives, mouths, and houses] by saying that the final [f, ɵ, s] of the underlying singular is replaced by [v, ð, z] before the bound form is added. The word 'before' in this statement means that the alternant of the bound form is the one appropriate to the substituted sound, thus, the plural of knife adds not [-s], but [-z]: 'first' the [-f] is replaced by [-v], and 'then'

> the appropriate alternant [-z] is added. The terms 'before', 'after', 'first', 'then', and so on, in such statements, tell the descriptive order. The actual sequence of constituents, and their structural order...are a part of the language, but the descriptive order of the grammatical features is a fiction and results simply from our method of describing the forms; it goes without saying, for instance, that the speaker who says knives, does not 'first' replace [f] by [v] and 'then' add [-z], but merely utters a form (knives) which in certain features differs from a certain other form (namely, knife). (1933, p. 213)

But that this position, apparently shared by Chomsky and Bloomfield, is not universally understood or accepted, is clear from the fact, for example, that Katz in his "Mentalism in Linguistics" bitterly criticizes exactly this quotation from Bloomfield, and provides himself such statements as the following:

> To explain how speakers are able to communicate in their language, the mentalist hypothesizes that, underlying the speaker's ability to

> communicate, there is a highly complex mechanism which is essentially the same as that underlying the linguistic ability of other speakers. He thus views the process of linguistic communication as one in which such mechanisms operate to encode and decode verbal messages.

This "encoding-decoding" bit, by the way, leads me to characterize this as the "secret agent" theory of language. Katz continues:

> The aim of theory construction in linguistics is taken to be the formulation of a theory that reveals the structure of this mechanism and explains the facts of linguistic communication by showing them to be behavioral consequences of the operation of a mechanism with just the structure that the formulated theory attributes to it. (1964, p. 128)

Outlining a description of speech production and recognition which exactly parallels the structure of a generative grammar Katz then informs us that:

> Within the framework of the above model of linguistic communication, every aspect of the mentalistic theory involves psychological reality. The linguistic description and the procedures of sentence production and recognition must correspond to independent mechanisms

in the brain. Componential distinctions between the syntactic, phonological and semantic components must rest on relevant differences between three neural sub-mechanisms of the mechanism which stores the linguistic description. The rules of each component must have their psychological reality in the input-output operations of the computing machinery of this mechanism. The ordering rules within a component must, contrary to the claims of Bloomfield and many others, have its psychological reality in those features of this computing machinery which group such input-output operations and make the performance of operations in one group a precondition for those in another to be performed. (1964, p. 133)

It is therefore clear that in unpacking Chomsky's metaphors as I have been attempting to do one is working against not only potential, but also quite actual, misunderstandings. A generative grammar is not, <u>per se</u>, a model of the mind, model of thinking or even of speaking. It is perfectly true that insofar as such

a grammar is linguistically adequate it will, as Chomsky notes, be reasonable to "incorporate it into" any such model proposed. If it is the production of more or less English sentences in which we are interested, then it will be reasonable to consider the best available characterization of English sentences. But it is essential to realize that such "incorporation" is needed only as a characterization of output and not as a characterization of outputter. It tells us what a "language production device" has to come up with, but not how it has to go about or does in fact go about doing it.

Chomsky is, of course, as the last quotation from him indicates, aware that this point has been misunderstood. He has even suggested that the term 'generative' as characterizing grammars of the sort in which he is interested might be abandoned in an effort to avoid such misunderstandings. I should like to suggest that it is not this particular term which is at fault, but the whole series of metaphors which compose the mentalistic myth in which generative grammars have been enshrouded and the mists of which I have been endeavoring in this paper to penetrate. The value of the contribution to understanding which the abandonment of this mythology would make is perhaps indicated above all by the fact that while, as should now be quite pellucidly clear, it is very difficult to

tell what in Chomsky is intended literally and what metaphorically, on turning from the question of performance to that of acquisition even Chomsky himself would seem to have been misled by his own myth. The same confusions of grammar with performatory model and knowing how with knowing that and still other and new confusions affect Chomsky's remarks about the "innateness" of language capacities and "Cartesian Linguistics." Comsky's use of terms such as 'innate-ideas,' 'rationalism,' 'empiricism,' 'induction,' and 'behaviorism' is, briefly, as idiosyncratic, as metaphorical, as is his use of the key terms already considered above. In the central senses usually given to these terms Chomsky is himself not a believer in innate ideas, is an empiricist, an inductivist, and even a behaviorist. Let us begin with behaviorism.

The term 'behaviorism' had already from the time of its inception and thus well before the generative grammarians came along, been applied in a sufficiently imprecise manner quite effectively to function as a source of confusion of things in themselves quite distinct. Chomsky and his followers are thus, even if guilty as charged, by no means the sole or original culprits in this regard. One sense of 'behaviorism' which might be clarified out of this historical confusion is that criticized above as based on a primitive conception of meaning. Behaviorists in this sense, believing that

terms to be meaningful must designate entities of some sort, and wishing to escape "mental" entities, were compelled to adopt the view that psychological terms necessarily designate, function as names for, chunks of behavior. In this sense of the term Chomsky is clearly not a behaviorist. It is, in fact, at least partly to signalize his rejection of just this sort of doctrine, his recognition that theoretical terms in linguistics need not designate pieces of behavior, that he has employed the term 'mentalism.' Now this usage is, as argued above, misleading. The behaviorism-mentalism dichotomy is valid only if the theory of meaning behind it is correct. Thus to reject "behaviorism" in this sense on the very good grounds Chomsky does, the grounds that not all psychological or linguistic terms do designate behaviors, is, as I have already indicated, to embrace "mentalism" only in a most unusually etherial sense of that term. It is true nevertheless that from behaviorism in this sense Chomsky, and generative grammarians generally, are free of all taint.

But this is, after all, a definitely old fashioned and distinctly outmoded sense of 'behaviorism.' The doctrine which most contemporary psychologists who consider themselves behaviorists denominate by that term is quite a different one. It is, moreover, a doctrine to which Chomsky himself has given an excellent formulation,

which he has explicitly recognized his holding in common with the most radical of the behaviorists such as B. F. Skinner, and to which he has, in fact, avowed an inability to conceive any alternative.

> Putting it differently, anyone who sets himself the problem of analyzing the causation of behavior will (in the absence of independent neurophysiological evidence) concern himself with the only data available, namely the record of inputs to the organism and the organism's present response, and will try to describe the functions specifing the response in terms of the history of inputs. This is nothing more than the definition of his problem. There are no possible grounds for argument here, if one accepts the problem as legitimate, though Skinner has often advanced and defended this definition of a problem as if it were a thesis which other investigators reject. (1959, p. 27)

The only change one might suggest here might be in the phrase "inputs to the organism." As Chomsky himself later points out, the problem of determining just what aspects of the environment do in fact constitute inputs or stimuli is itself one of the key problems the investigator must solve. It might thus be better to frame the

program somewhat more neutrally as simply one of finding functions connecting features of the organism's environment past and present with its behavior. With this slight modification, however, Chomsky's statement could hardly be improved upon as a formulation of the program of behaviorism in the currently most generally accepted sense of that term. I would tend to agree with Chomsky that any alternative to this program is, in a sense, inconceivable. But it does not follow from this that those who, like Skinner, have argued for it were therefore wasting their time in so doing. It is also in a sense inconceivable that one could trisect an angle with straight edge and compass. But the number of people who have tried makes proof of its inconceivability very helpful indeed. The number of Cartesian linguists who have thought they could provide some alternative to the behavioristic program is likewise sufficient to make proof of the inconceivability of such alternatives helpful as well.

The "behaviorism" Chomsky rejects, then and the "mentalism" to which he opposes it must both be seen as simply subdomains within behaviorism in this broader and more basic sense. The above quotation continues:

> The differences that arise between those who affirm and those who deny the importance of the specific

'contribution of the organism' to learning and performance concern the particular character and complexity of this function, and the kinds of observation and research necessary for arriving at a precise specification of it. If the contribution of the organism is complex, the only hope of predicting behavior even in a gross way will be through a very indirect program of research that begins by studying the detailed character of the behavior itself and the particular capacities of the organism involved. (1959, p. 27)

Here the phrase 'importance of the specific contribution of the organism' may, as Chomsky's use of scare quotes indicates, mislead. Sticks and stones do not behave at all well in Skinner boxes. The "contribution of the organism" is in every case essential and it is difficult to get more importance than that. What Chomsky's "mentalism" amounts to in this context, then, is simply the hypothesis that the functions connecting verbal behavior with environmental factors will in most cases be complex, may differ from any involving solely non-verbal or at least non-human behavior, and, presuming that 'organism' means organism rather than species, may even vary significantly from individual to individual.

Even this is not sufficiently precise. Those, and there would seem to be many, who that think that in the review from which these quotations have been taken or elsewhere Chomsky has shown a Skinnerian type of conceptual framework of stimulus, response, reinforcement and rest to be somehow inadequate in principle to cope with verbal behavior have been grossly unfair to Chomsky as well as to Skinner. Such people have simply not read Chomsky carefully enough to do justice to the care, precision, rigor and avoidance of hyperbolic claims his actual argument displays.

What Chomsky has pointed out, and correctly pointed out, is the vast extent of our ignorance of the variables and functions actually involved in verbal behavior, "how little is really known about this remarkably complex phenomena." His entire case against Skinner consists in pointing out that this factual ignorance is not alleviated in the slightest simply by the introduction into it of a new conceptual structure. The only thing which could remedy this ignorance is actual empirical studies which neither Skinner nor anyone else has yet sufficiently provided. In this sense, then, Skinner's Verbal Behavior does nothing to explain verbal behavior, and may even, insofar as it masks our actual ignorance in an elaborate display of apparently scientific terminology, obstruct understanding.

Even this is probably not being quite fair. We do not know very much about the factors and functions involved in verbal behavior. Skinner does not provide us with this knowledge. In this sense his key concepts are indeed empty. We can talk of stimuli and responses and contigencies of reinforcement, but with verbal behavior by human beings, as opposed to bar pressing behavior by rats, we do not yet have enough knowledge to tell to what specifically these terms are to be applied. In this sense Chomsky is quite correct in calling Skinner's employment of these terms "metaphorical" and at best merely equivalent to our ordinary terminology. Yet there is another sense in which the two terminologies are not equivalent. The real advantage of the Skinnerian formulations is just exactly that their nakedness is so very obvious, that, unlike our ordinary formulations, they make it so very clear that and what we do not know. This is the case since their form, as opposed to that of our ordinary locutions, makes so very easy their contrast with other situations, such as bar pressing by rats, in which we do have a fairly good knowledge of what is going on.

We can be sure in any event that the knowledge of verbal behavior we lack is not going to be supplied simply by the adoption of a "mentalistic" vocabularly anymore than simply by the adoption of a "non-mentalistic" one, and that the Skinnerian terminology at least frees us from certain temptations inherant in "mentalistic" alternatives. One example only. In this same review Chomsky

points out that one might readily form an hypothesis as to how the development of the gaping response of a nestling thrush developed through differential reinforcement but that there is good evidence that these responses actually develop simply through genetically determined maturation. Stressing that the development of a child's imitating new words is in many respects parallel to that of the thrush's behavior Chomsky suggests that it too could conceivably be largely simply a matter of maturation. "To the extent that this is true, " Chomsky concludes, "an account of the development and causation that fails to consider the structure of the organism will provide no understanding of the real processes involved." (1959, pp. 43-44) But to say that the development either of gaping or of imitative behavior is a product simply of maturation is not to say anything at all about the structure of bird or child except in the completely trivial sense that the structure is such as to mature in this way. The study which would lead to such a conclusion is not an anatomical but rather a behavioral one. To make such statements about the development of behavior through maturation is rather to relate the behavior to a set of environmental conditions of which its development is, and particularly is not, a function--exactly the sort of question

Chomsky characterized in the above quotation as the only kind conceiveable.

In consideration of all of these facts one can hardly conclude other than that statements to the effect that the results of modern linguistics demonstrate a "mentalism" and are inconsistent with "behaviorism" are somewhat over-simplified at best. But so is it also with "rationalism," "empiricism," "innate ideas" and "induction."

The definitive issue between rationalists and empiricists over innate ideas and induction has been a matter not of acquisition or origin at all but rather of validity. "Nothing in the mind that was not first in the senses" has actually functioned basically not as a bastard psychological hypothesis but rather as an attempt to assure empirical applicability and validation. Neither classical nor modern empiricisim has held that either concepts or, a fortiori, judgements, laws and theories are somehow simply imposed upon us by objects in the world. Even raw sensations themselves have been universally recognized as arising from at least the interaction between objects and ourselves and as possessing characteristics and structurings dependent at least in part upon us and our characteristics and structurings. Both concepts and theories are generally regarded as being, in Einstein's splendid, if less than completely informative phrase "free creations of the human mind." The distinctive position of the

empiricist consists solely in insisting that in so far as concepts and theories are to be regarded as being empirical, as being about the world, so far must they be tested for the adequacy with which they do in fact cope with the data, do in fact succeed in organizing our experience.

When Chomsky says such things as, of the child, that "His knowledge of the language, as this is determined by his internalized grammar, goes far beyond the presented primary linguistic data and is in no sense an 'inductive generalization' from these data," (1965, p. 33) he is apparently using the term 'induction' for what is ordinarily called 'complete induction' or 'induction by simple enumeration,' for induction in which the "sample" is the entire reference class. This is, of course, one sense of 'induction,' but a limited, derivative and trivial sense indeed. By 'induction' in general and without qualification is meant just exactly such an extension beyond the given data of an hypothesis which works for them. If it were not for this extension there would be no problem at all to the famous "problem of induction."

More significantly, however, much of what Chomsky has to say about "inductivist, empiricist theories of language" even in the normal sense of 'induction' is quite correct. These are not good, or at least not complete, theories of language acquisition. What Chomsky's idiosyncratic usage conceals is that the reason for this is not that they are bad theories of language acquisition, but rather

that they are not really complete theories of language acquisition at all. Because the issue is one of validation rather than of origin "induction" is at best a very partial explanation of the acquisition of anything. To say that something, linguistic or otherwise, has been learned by induction is to say that concepts and hypotheses have been tested by experience, but it is to say nothing whatsoever about how these concepts and hypotheses have been developed in the first place so that they could be so tested.

Interpreted literally Chomsky is simply talking nonsense in statements such as the following:

> In general, then, it seems to me correct to say that empiricist theories about language acquisition are refutable whenever they are clear, and that further empiricist speculations have been quite empty and uninformative. On the other hand, the rationalist approach exemplified by recent work in the theory of transformational grammar seems to have proved fairly productive, to be fully in accord with what is known about language, and to offer at least some hope of providing a hypothesis about the intrinsic structure of a language acquisition system that will meet the condition of adequacy-in-principle and do so in a sufficiently narrow and interesting way so that the question of feasibility can, for the first time, be seriously raised. (1965, pp. 54-55)

In the normal acceptation of the terms there is and can be no such thing as an "empiricist theory of language acquisition" unless by this one means a theory which has been or is to be tested empirically. Nor can there be a competing "rationalist" theory unless by this one means a theory deduced by pure reason from some kind of a priori principles. On this interpretation the type of theory Chomsky proposes would be doubly empirical and not in the slightest way rationalist. Such theories would themselves be perfectly good empirical theories, and the procedure they envisage the learning child going through, involving as it does rejection, on the basis of experience, of grammars of languages other than that he is learning, would be perfectly good empirical procedure.

What Chomsky actually means by such statements, on the other hand, is quite well taken if somewhat less exciting than what he says. Unless we regard as an acquisition theory the methods developed by taxonomic linguistics for relatively mechanically developing grammars from given bodies of data, methods which were certainly never intended as such a theory and which function rather unhappily when thrust willy-nilly into that role, there simply are no detailed theories of language acquisition. Competing theories cannot, therefore, serve as any kind of ground for resisting the development of theories involving inherent and species specific mechanisms nor for rejecting such theories should they be developed--providing they meet the empirical tests

The only objection one could raise to this is to suggest that it is too good to be limited to linguistic behavior but could be applied to any or all of the vast array of behavior more or less specific to the human species. There is nothing in any of this which implies, as MIT seems sometimes to infer, a sharp distinction between such other behaviors and the linguistic.

Nor does any of this pertain particularly to generative grammars. The basic point Chomsky seems to overlook in his discussion of acquisition is that since our generative grammars do not give us models or theories of performance, they do not actually give us models or theories of what is acquired. The competence metaphor is undoubtedly one of the factors causing confusion here. Chomsky's idiosyncratic use of the term 'competence' makes it harder to see that competence in the ordinary sense would be competence to do, i.e. to perform. Our generative grammars characterize what is put out but not the way in which it is put out although it is actually the latter which actually acquired.

Thus Chomsky tells us you will recall, in the passage initially quoted, that:

> The problem for the linguist, as well as for the child learning the language, is to determine from the data of performance the underlying system of

> rules that has been mastered by the speaker-hearer and that he puts to use in actual performance. (1965, p. 4)

But, one final time again, this cannot be taken literally. "The speaker-hearer" to the products of whose performance our generative grammars would actually apply does not exist it is to be recalled. The phrase "that he puts to use" would imply that our generative grammar does indeed provide us with a theory of production, of how, or at least partly how, speakers actually go about producing sentences. But, as we have seen and as we have seen that Chomsky has seen, this is not at all the case. The problem for the linguist, then, is not at all that of determining <u>the</u> system of rules, but rather <u>a</u> system of rules more or less adequate to the data and to such other constraints as simplicity, consistency, non-redundacy, effectiveness and so on which we may impose on them. The problem for the child is still less one of discovering <u>the</u> rules, but is rather one of learning to come up with--in whatever way he can--linguistic behavior more or less like that of the rest of us.

This identification of the learning child and the working linguist which looms so large in Chomsky's exposition, although hardly in the substantive portions of his work, is thus surely whimsical at best. Such an identification, in addition to overlooking the non-congruance I have emphasized between grammar and

performance, also overlooks the very substantial differences between knowing how and knowing that, differences again exactly comparable to those between learning to walk or to catch a ball, on the one hand, and formulating theorems of mechanics on the other. Let me cite only one example of the sort of confusions to which such an identification leads. Chomsky as a particularly ingenious and imaginative linguist usually finds available to him a multiplicity of grammatical hypotheses each of which is compatible with any given finite body of linguistic data. He therefore needs principles of selection or justification for for choosing among these. He therefore assumes that the child learning the language must have similar principles of selection. But clearly, even ignoring for the moment the differences between grammar and performance and between what one might call a "physical" as opposed to an "intellectual" performance, for the child, as opposed to the linguist, anything that will do the job is actually quite adequate. The child has merely to speak and understand as best he can. He does not have to defend to his colleagues or even to himself the "principles" by which he does so.

Generative grammars, insofar as they provide better characterizations of languages than do alternative types of grammars, will certainly be relevant to theories or models of language acquisition. But they cannot themselves provide or directly contribute to such models any more than, and in fact even less than, they can provide models of performance.

These considerations lead naturally to the question of explanation in linguistics. As far as I am aware the only ground ever explicitly offered for taking the mentalistic myth literally is that unless we do so linguistic results are not and cannot be explained. It is therefore essential for an evaluation of literal linguistic mentalism to consider this aspect at least briefly.

Chomsky has on occasion distinguished three "levels of success" for grammatical description. The first and lowest of these he has characterized as merely presenting correctly the data of performance in his sense of that term. The second and higher level is supposedly achieved when a correct account is given of linguistic intuition. The third and highest or explanatory level is achieved when a basis is found for selecting a grammar that achieves the second level over alternatives which do not. (1964, pp. 62-79) Now I have already noted that this way of describing the distinction between "levels" one and two is most misleading. Chomsky in fact himself abandons it in the later Aspects, there distinguishing only between descriptive and explanatory adequacy. (1965, pp. 24-27) In actuality, however, the original three-fold distinction which Chomsky had in mind, while not at all what he described, is as it emerges from his practice an entirely reasonable one.

Consider an example Chomsky gives us from phonology. There is an English word 'pick' but there is not or at least was not until it

was introduced by R. M. Hare, a word /blik/ nor is therea word /ftik/. The first level of adequacy would then be attained by an English grammar that contained a lexical rule introducing /pik/ but not /blik/ or /ftik/. The second level of adequacy would be attained by a grammar that contained in addition a general rule excluding /ftik/ but not /blik/. The third level would provide a ground for including this latter rule but excluding the factually correct "rule" that in the context #b_ik# a liquid is necessarily /r/. What Chomsky actually has in mind, in short, is a simple list as opposed to a neat calculus as opposed in its turn to a broader theory containing this. But there is nothing mentalistic in all this. /blik/ is the sort of thing that does occur in English even though it itself specifically may not, and not only does /ftik/ not occur, but nothing else of that kind does either. It is just this sort of occurrence and non-occurrence of kinds in the objective data which even our third level of theory needs to attain--and not some obscure occurrence or structure in the nethermost regions of the soul.

Consider an example from syntax. Take an array of English sentences and non-sentences such as the following which illustrate similarities and differences in functioning between the word 'find' and the word 'be': ('John found the book' - 'John was a farmer.'), ('the book was found by John' - *'a farmer was been by John') and so on.

One could then merely list these differences thus attaining the first level of adequacy. Chomsky can and does, however, give five simple rules from which all the sentences and none of the non-sentences can be generated thus attaining the second level of adequacy. These rules moreover can be justified or explained, thus attaining the third level of adequacy, by the entirely practicable proof that they would have to be complicated considerably before they would generate the non-sentences.

This latter type of explanation or justification is the kind most frequently used in actual linguistic practice. Chomsky's preferred method of explanation, that in terms of linguistic universals, is not often actually employed if only because if there actually are any linguistic universals we know very little of them at the present time. The essential thing to see here, however, is that neither of these kinds of explanations involves anything whatsoever that is mentalistic. The one is largely in terms of formal logical characteristics, the other in terms of more specific regularities within broader regularities. Explanation of grammars as it occurs in actual linguistic practice, then, like grammars themselves owes nothing whatsoever to the myth of mentalism.

Still in all, one may think--and some like Katz have not only thought but said--such "explanations" in terms of mere brute regularities or formal characteristics of rules are not really explanations at all but still mere descriptions. In order really to explain

grammatical results it is essential to hypothesize that they actually duplicate, are actually isomorphic to, actual neural structures in the brains of actual speaker-hearers. Without this hypothesis there is a gap in the chain of causality and thus in our explanations. But the response to this is obvious and decisive. One does not explain by hypothesizing. If physiologists were to discover that and how neurological mechanisms produced speech acts, whatever this might mean, then they might be said in one sense at least to have explained these. But one certainly does not explain or justify grammatical rules simply by supposing, as I for one certainly should, that they might have some sort of physiological basis, nor yet by supposing that this basis might somehow be precisely isomorphic to the grammar explained--just or even approximately how being left quite obscure.

To say this is not to say, on the other hand, that theoretical constructs in linguistics or elsewhere are "fictions." Those who have used this term were themselves speaking metaphorically. No one has ever actually thought that the atom has <u>just</u> the status of Mr. Pickwick or Hamlet. To be a theoretical "entity" in a successful scientific theory is as different from being a character in a successful play or novel as the criteria for success are different in the two cases. Nor, since the use of such theories is itself a part of what we mean by "explanation," should we argue that such a theory cannot "really explain" unless its theoretical terms function more or less as do the names of cats or dogs.

Now of course nothing I have been saying is intended to or should suggest that one could not use a grammar as the basis for formulating psychological or perhaps even physiological hypotheses. One could also use the crossword puzzle in last Sunday's Times as such a basis for that matter. But it is important to realize that formulating such an hypothesis from a grammar is by no means an easy task. Grammars, even generative grammars, are not ready-made psychological and still less neurological hypotheses the myth of mentalism notwithstanding. This should be quite clear to anyone who has more than glanced at the various attempts to test the "psychological reality" of various grammatical elements and the numerous and profound difficulties encountered in these attempts. The even more formidable difficulties involved in deriving from grammars anything like meaningful neurological hypotheses have not even been approached except perhaps by those philosophers who have struggled with the mind-brain identity theory and in doing so come up with far more problems than solutions.

It is perhaps more important to realize that even if such psychological or neurological hypotheses could be formulated, they would be psychological or neurological hypotheses and not linguistic ones. Suppose we are playing the parlor game of "John is ..." This consists of trying to think of and state as many characteristics of John as possible. The one whose turn it is and can't think of another characteristic of John loses. We have been at it for half an hour now and are running out of ideas. "John is six feet tall," I say

"John is eager to please," you retort. "John is ..." I pause searching, and then it comes: "easy to please, " I conclude. Suppose now, as seems quite reasonable, that what I have actually thought is just exactly what I have actually said, that in my thought "John" is the subject and "easy to please" is as fully a predicate, a characteristic of John, as is "eager to please." Is this to be considered any kind of evidence at all against Chomsky's assertion that while 'John' in 'John is eager to please' is the logical subject of 'please,' 'John' in 'John is easy to please' is the direct object? Chomsky's statement is a grammatical one which must comprehend such factors as that 'John is easy to please' is approximately equivalent to 'It is easy to please John' while 'John is eager to please' is scarcely so to 'It is eager to please John.' But in my game I, as opposed to Chomsky in his, need not have thought of any of this; it need never have entered my mind. So too with the fact cheerfully cited by Chomsky himself that in such expressions as 'This is the cat that caught the rat that stole the cheese,' the intonation breaks are ordinarily inserted in the "wrong" places. That such expressions are not divided in speech and thought in the way Chomsky or any other reasonable grammarian would divide them cannot be any stretch of the imagination be taken as evidence against the adequacy of Chomsky's grammatical formulations. The only casualty of such facts is the myth of mentalism.

The contributions of MIT linguistics when shorn of myth can

be seen to accord quite precisely with those conceptions of science formulated, discussed, understood and accepted by the vast majority of contemporary empirically minded philosophers of science. As such these contributions constitute not a challenge but a confirmation. For the psychologist and the philosopher of mind these contributions are of interest in just the way any accurate grammar is as a type of characterization of certain types of behavior. Insofar as generative grammars are more accurate representations of these phenomena than are alternative formulations they will presumably be that much more interesting. But here again these linguistic theories themselves do not and cannot constitute a challenge. For they are not themselves psychological theories and still less philosophies of mind. Linguistic theory, however high-powered, is still just linguistic theory and the constructions of imagination wrought by grammarians, however necessary and natural they may appear to their discoverers, are not given realities mental or physical but constructions of the imagination still.

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